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THE AUTHORITY OF THE SCIENTIFIC REJECTION OF PSEUDO-SCIENCE

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Introduction

This paper is organized around the question, 'On what authority can competing systems of belief and practice be rejected by scientists as pseudo-science?' In the modern philosophy of science which descends from the Enlightenment, it is often taken to be a platitude that science and religion are polar opposites. The authority of science stems from rigorous reasoning based on disciplined observation, while the authority of religion depends upon an institutionalized tradition which controls the interpretation of sacred texts and religious experience. In the context of a discussion of tradition and authority in science and religion, however,(1) it would seem to be natural to regard science and religion as parallel long-lived traditions, each of which provides legitimacy for the institutional structures through which authority is imposed. In this context it becomes appropriate to explore the analogy between the scientific rejection of pseudo-science and the way religious traditions once identified and dealt with heresy. However, if science is to be regarded as a tradition, it must be recognized that it is a rather special kind of non-conservative tradition which differs radically from usual views of religious traditions.

Against the background of the rivalry between these two perspectives, particularly as it applies to the way we seek to legitimate the rejection of radical alternatives as pseudo-science, this paper seeks to bring out some inherent problems in the nature of our conception of science. It will be argued that there is a tension within philosophy of science between the claims that scientific knowledge is supremely reliable and that it is exceptionally receptive to revision. By being so reliable, scientific knowledge gains a distinctive authority, but by being so open to modification, the scientific authority is inevitably undermined. The conflict between these ideological uses of philosophy of science carries through into other approaches to science and has a general bearing on how we regard the relationship between science and religion.

The Authority of Scientific Rationality vs. the Authority of Religious Tradition

The story is familiar of the growth of the view that science rejects authority based merely on tradition, employing instead rigorously formulated (often mathematical) reasoning based on disciplined systematic observations (or *a priori* insight). This view gained wide currency in association with epistemological traditions which grew out of the sceptical individualistic question, 'What do I know?' Although seventeenth century prophets, pioneers and apologists for natural philosophy sought to intertwine the foundations of science and religion, giving a special epistemological status to religious knowledge, this stood in clear contrast to their scathing rejection as pseudo-science of speculative traditions of astrology, alchemy, magic and mysticism. In the wake of the scientific revolution, Enlightenment philosophers claimed that natural philosophy provides a model for knowledge based on experience. They sought to demystify forms of belief based on custom by exposing them to the critical appraisal of common sense. The critical rejection of mere tradition was increasingly applied to the prevailing forms of religion as well by many Enlightenment philosophers and their nineteenth century successors. Enlightenment philosophy celebrated

the authority of reason in place of the authority of tradition. Nineteenth century positivism developed further the sense of opposition between primitive forms of religious thought and the more rigorous but also more restrained positivistic thinking which characterizes modern science. By the end of the nineteenth century, science as represented in positivism was offered not merely as more authoritative than pseudo-sciences such as astrology or phrenology but also as a direct replacement for religion. The notion of the warfare of science and religion gained wide currency. I will call the post-Enlightenment tradition of philosophy of science within which positivism stands as an extreme form, 'main stream philosophy of science.'

An initial qualification is required for the conception of science as gaining its special authority from its rationality. As science became an institutionalized form of research practice, the idea that it is the most authoritative form of knowledge was combined with the conception that it is the most effective procedure for producing knowledge. In the early nineteenth century, several accounts of scientific method were formally set out, for example, the positivist and inductive methodologies. Until the twentieth century, the conception of science as both authoritative and changing seemed coherent because scientific change was regarded as cumulative and progressive. There was no need to concede that any scientific finding might be wrong simply because further discoveries were to be expected. From a late twentieth century perspective, this might seem naive, at least for the authoritative application of science. A similar modern example is the recognition that although an early pesticide such as DDT was effective at killing virtually all the insect pests to which it was initially applied, as its discoverers claimed, we now know that DDT also kills the insect predators of those pests, together with life forms in the waterways into which the DDT eventually finds its way and those high in the food chains based on the creatures directly affected. We would now think that the authority of the original knowledge has been undermined merely by the recognition that it is *partial* knowledge. It is not adequate for the intended scientific application unless independently produced knowledge is added. (The only error in the original cognitive claim in its own terms was the failure to anticipate the emergence of insecticide resistance.)

The authority of scientific knowledge has been further weakened in the twentieth century by recognition that not merely may knowledge be *extended*, but existing insights may be *displaced* by new and different theories in the normal process of scientific change. K.R. Popper's emphasis on the fallibility of science puts this feature into focus.(1) It has become a commonplace to stress that science is *critical*, that it does not arrive at firm conclusions, and that its status as science depends on the institutionalization of receptivity to changes in current ideas.

We see, therefore, that in the main stream philosophy of science, there is a tension between the urge to make it the supremely reliable form of experiential knowledge and the urge to proclaim it as the most progressive form of empirical inquiry. This tension is reflected in the use of the concept 'scientific knowledge.' Presumably, if any form of experience deserves to be called 'knowledge,' science does, and yet the claim that present-day scientists are critical of their knowledge-claims and subject them to radical revision has the implication that, as in the history of previous science, much of what is now regarded as scientific knowledge will be abandoned in the future. As clear illustration of this tension, consider Ziman (1978). This book seeks to emphasize the exceptional reliability of science, as part of an argument that scientific knowledge deserves a high level of authority. But Ziman feels obliged to include a small section, 'How *much* can be believed?,' in which he comes to the conclusion that because it is in the province of negotiation and renegotiation by experts, there is no hard and fast criterion for the outsider as to what is to be believed in science. This retreat into the mystique of professionalism looks like an attempt to evade the issue.

On the view that scientific knowledge is subject to change, it is inappropriate to justify the scientific rejection of pseudo-science on the grounds that it is inconsistent with present orthodox science, because further research may show that present science is actually untrue. The rejection must instead be grounded on the claim that the pseudo-science employs defective procedures for generating and evaluating its conclusions.

Parallel Traditions

As the centuries have gone by since the scientific revolution, the cumulative continuity of science has increasingly made it as much of a tradition as religion. It shows considerable continuity in the development of its institutional structures, each generation undergoing an extended training in which the skills of its elders are handed on.

Over the last few decades, the development of professional history of science has facilitated the emergence of the intellectual tools to explore a conception of science as a tradition. M. Polanyi emphasized the

importance of the tacit components of scientific knowledge in the practice and judgement of scientists, and saw the authority of science as depending crucially on its tacit knowledge.(2) Similarly, T.S. Kuhn gave this orientation to the study of science a major boost beyond its natural base in the history of science. M.D. King (1971), in a discussion of the epistemological and sociological significance of Kuhn, set out the stark contrast with the earlier approach. "Positivism treats tradition and science as natural enemies. Traditional beliefs and practices exercise a magical power over men which protects them from the critical appraisal of common sense. Science demystifies tradition; it penetrates the enchanted defences of tradition and exposes its cognitive and technical absurdities." In contrast, King described Kuhn as considering science in terms of local traditions, asking, "What is the nature of the authority exercised by or through such paradigms? How is it exercised? And how does it come to be overthrown?" (King, 1971, p. 21). Since Polanyi and Kuhn, sociology of science has come to be prominent among the approaches which represent science as a social tradition. Sociology of science frequently treats scientific knowledge as nothing but the primary cultural product of that tradition.

I wish to support the view that modern science constitutes a tradition. But science is not a conservative tradition. The stable structures which continue are not systems of knowledge, handed on from generation to generation. Scientists are not committed to an unchanging body of knowledge or rules of practice but rather to what holds their social system together. The stability lies in the institutional form of science and the sustained wider social toleration of and support for the institutionalized practice. The cognitive authority of science comes, not from the traditional firmness of the conclusions it provides about the world, nor from the immediate checkability of their anchorage in direct observation. Rather, it comes from the collective influence of mutually reinforcing judgements of elite experts.

The Role of Observation in Scientific Tradition

The rise in importance of regarding science as a social tradition as a corrective to purely rational representation is appropriately illustrated by the changes in the status of observation as the foundation of scientific knowledge.

Early modern science shared the prevailing individualistic epistemology of its time. In the epistemology of Locke, which became a widely recognized synthesis of the knowledge base of the new natural science, each individual makes his own cognitive judgement based on his own observations and his own reasoning. To the extent that scientific knowledge produced a consensus, it was because anybody seeing the same things without illusion and avoiding errors in their reasoning would be led to the same conclusion. If a person resisted the standard conclusion in spite of making the appropriate observations, others could try the coercive pressure of rational argument, and if that failed would finally judge that that person did not deserve to be taken seriously -- perhaps justifying their rejection by saying that he was mad or bad. This theory of science only arose as the outcome of extended discussion and disagreement. The new natural philosophy was, for example, attached to an elitist social establishment. S. Shapin and S. Schaffer (1985) have argued that Hobbes vigorously resisted Boyle's version of scientific knowledge as based on open experimental demonstration. For example, they point out how Hobbes complained that Boyle's experiments were *not* open to all, and those who did see, were constrained to see them in Boyle's terms. However, in that debate, it was Boyle's rhetoric which became established, and which Locke's epistemology reflected, not Hobbes's. Even the more sceptical of Locke's philosophical successors retained the general framework of his epistemology.

Since the seventeenth century, the only acknowledged role for tradition in the dominant philosophical representation of science was as the mechanism of transmission of individual persuasion through which each person is exposed to appropriate personal experience and shown the appropriate chains of consequential reasoning. But in the late twentieth century, the situation is very different. First, modern science is far less plausibly constructed out of direct observation of the primary qualities of things as Locke argued in the seventeenth century. The situations observed in the practice of science are increasingly artificial, requiring highly skilled training for their construction. The senses employed in making observations are increasingly augmented by sophisticated instrumentation, so that we see the world differently. The observational statements produced are expressed in theory-based language. The judgements contained in reports of observations require skilled technical reasoning. So it has become decreasingly plausible to regard the world experienced by the scientist as being mediated by pure sensation. Secondly, modern sciences are now far less dogmatic in their conclusions. Conclusions of the present day may be replaced by others in the future, so it must always be legitimate for individuals to challenge

ideas of the present. Thirdly, modern sciences are so complex and so highly elaborated that no one person can understand more than a tiny proportion except in the most superficial way. As a result, science is practiced by narrowly trained professionals who produce conclusions which only the relevant experts are competent to judge. The experience which comes into scientific conclusions is the trained observation of experts who draw upon specific traditions, upon the tacit knowledge of rich and relevant experience and upon technical forms of reasoning to reach conclusions that can only be effectively challenged by outsiders who have access to equally authoritative sources of specialist knowledge. If in the present day, a conflict arises between the claims of science and a rival system of unorthodox understanding, such as, say astrology, it can be quite difficult to disentangle the precise route from observation through reason to conclusions which rationally authorize the rejection of the unorthodox rival.

Since, in the classical scientific epistemology, judgements of knowledge were to be left to the individual, problems arise, as Hobbes noted, if an individual cannot be put into a position in which his own observation and thinking will logically drive him to the correct judgement. As it is the experts who make the observations and judgements in modern science, the nonexpert can only be persuaded by accepting the word of the experts. Furthermore, an individual who knows how to play the game of rational argument and rational evasion can draw on a rich culture of ways of *avoiding* the conclusions of the scientific experts. The individual who is prepared to challenge the experts will tend to be lost from the rationally based consensus. So modern science, when combined with individualist epistemology, cannot rationally coerce any individual into agreement so easily.

The change in the status of observation is readily put in a paradoxical form by noting the relative change in the status of observation in orthodox and unorthodox science. While orthodox science has become more like a social tradition, the opposite trend has been going on in the unorthodox rival practices which the orthodoxy rejects as pseudo-science. The modern practitioners of such alternative practices as astrology typically appeal to the intuitive judgement of the user based on his own experience. That is to say, the modern unorthodox sciences appeal less to the antiquity of their tradition than they do to the personal experience of the those tempted to dabble in their ideas. One popular book on pyramids claims that if only you will try the experiment of putting a razor blade under a pyramid you will *see for yourself* that it remains sharp indefinitely (Toth and Nielsen, 1976). Enthusiasts for astrology tell me that the best test of astrology is to run your life according to the advice in horoscopes. The difference between scientific orthodoxy and unorthodoxy is put especially nicely by the contrasting attitudes to experience of psychic phenomena. If a spoon is bent by gentle stroking while people watch, the orthodox scientist is liable to pronounce that because of the conflict of the idea of the paranormal with scientific orthodoxy, in situations like this, one cannot believe what one sees. He may say that there must have been some sort of trickery, even before he is sure what it is. In contrast, the unorthodox scientist is much more likely to say, 'seeing is believing,' and conclude that a prima facie case has been made for the existence of paranormal powers. There may seem to be something paradoxical in this inversion of the role of personal experience in science and pseudo-science over the centuries. It may appear less surprising when one considers the importance for pseudo-sciences to retain close connection with the human scale of experience and values if they are to gain patronage within popular culture. The institutions of scientific orthodoxy need only superficial popular appreciation and can therefore more easily afford to move away from the human scale.

The Parallel Between Science and Religion in Coping with Unorthodoxy

We might consider, therefore, the treatment of science and religion as comparable traditions. This would suggest, for example, that there should be a parallel between the way the pseudo-sciences are dealt with by established science and the way heresy was once handled within established religion. Both science and religion constitute tradition-based social practices, defenders of which have an interest in maintaining social control over any proposed variations. However, neither science nor religion is adequately represented as an explicit body of static knowledge and practice. Clearly, the implicit aspects of the tradition must be relearned by each new generation. Both science and religion include informal skills which each individual must acquire for himself through the example of others, through personal experience and through inspiration. Thomson (1987, 1988), for example, emphasizes the parallel between the tacit knowledge of research skill and that of the religious search for personal insight by acts of prayer and worship in arguments that follow Polanyi's line quite closely. Each individual may represent this process to himself and to others in a unique way. In a thriving tradition, the communication of such insight and understanding will tend to cumulate. Practitioners of either orthodoxy are disposed to argue that there are procedures by which new

insights may be accepted as legitimate and assimilated by the tradition. We might characterize heretics and pseudo-scientists as those who have attempted to evade the control procedures in a way which puts at risk the cumulated wisdom and practice of the tradition. Established interests are defended by such an ideology of exclusion, with its appeal to the privileged nature of the knowledge carried within the orthodox tradition.

In a tradition which is open to change from within, those new insights and practices which gain social approval cumulate and eventually lead to institutional changes. In the case of science the normal process is part rational and part social. Individual practitioners are expected to produce and to justify extensions and elaborations of previous understanding. This must be done in approved ways, the conventions for which require a procedure of objectified evidence and argument, which other people can check for themselves. Philosophical versions of the process focus upon institutionalized procedures of social control. Since ambitious scientists make their reputation by the originality and verve with which they can produce novel extensions and persuade others to accept them, there is always a risk that an individual will be too bold. The nature of this risk depends to some extent on one's status in science. The elite of science -- those who are most productive and most visible and in fullest contact with other well connected people -- have a greater freedom to depart from the existing rules, for in a sense, whatever they do is permissible as science, even though it may not finally be accepted, simply because it is done by the elite. In contrast, there is likely to be less tolerance of lower ranking figures in orthodox science who vary standard practice in other than the most routine ways. And those who are outside science are sure to be ignored unless they play the game superlatively well by the rules of established science. All proposed variations of science which have not survived critical exposure may subsequently be ignored or rejected, and if their supporters continue to advocate them, may be labelled as pseudo-science.

The processes of change in modern religions also builds upon a separation between the achievement of individual understanding and adoption of new doctrines and practices by church hierarchies. There are recognized procedures for the institutionalization of change and the social position of those promoting a change is at least as important as the objective nature of the arguments employed. Although the word 'heresy' is not used very often in the modern situation, people are still worried about the kinds of extreme variants of individual religious belief which would have been called heresies in the past. The analogy with pseudo-science would suggest that the label would be applied especially to attempts to build a social following for unorthodox beliefs and practices within a church. However, in the modern context of religious pluralism, it would seem to be relatively easy for the unorthodox group simply to leave one Church and found another.

Difficulties with the Concept of Scientific Knowledge

The situation the preceding discussion has set up is one in which middle-of-the-road conceptions of science accept that it is a social tradition, but also wish to defend the mainstream philosophy of science presumption that science gives knowledge of reality by the combination of observation and reason. The issues I wish to discuss have become blurred by the layer upon layer of revision of the concept of knowledge in successive discussions. Whatever the term meant in ordinary usage was modified by classical philosophy. The sceptical tradition of modern philosophy with its denial of innate knowledge and its doubts about empirical knowledge introduced a different set of standards, undermining the application of common-sense ideas of knowledge to the outcome of the scientific process. These standards were flouted in mainstream philosophy of science with the idea of inductive knowledge, in a context which debated the problem of induction. Modern critical philosophies of science such as that of Popper, with their open recognition that present scientific ideas may be wrong, have modified the concept of scientific knowledge further. The phrase 'scientific knowledge' continues to be used, even though we recognize that the very activity of science makes the scientific conclusions of an earlier time out of date. Perhaps in such a context the further modification of the concept of knowledge to a purely descriptive concept by sociologists of scientific knowledge is defensible. The label is typically applied to transient foci of belief which a social community represents to itself and others as knowledge.(3) The term 'scientific knowledge' has, then, acquired such a diversity of uses and qualifications that it can mean almost anything to anybody. I wish to suggest that we can gain a new perspective upon the underlying issues by focusing on a different but related term, 'belief.'

Belief in Religion and Science

From the point of view of the individual, a vital aspect of religion is belief. Indeed, in the face of scepticism, a committed individual member of a religion

is asked to hold beliefs out of faith. So 'belief,' 'faith' and 'commitment' are words that are closely linked. Very often religions have specific beliefs that believers must commit themselves to as articles of faith. More generally, there is a vague constellation of belief which is a necessary condition of belonging to a religion. Perhaps some Christian denominations do not require such commitments any more. But surely, to the extent that that is the case, they are less like religions and more like the residual institutional shells of religions.(4)

In contrast, the individual practitioner of modern science is not required to believe any specific propositions in order to occupy the role of scientist. (He may, however, be expected to believe in the general effectiveness of scientific procedures and the collective competence and integrity of the scientific community.) Although no specific propositions must be believed by a card-carrying physicist or psychologist, there might be requirements of belief for belonging to a particular school within science (for example, to be an authentic behaviorist within psychology), but those excluded by lack of the appropriate commitment of belief from such a school can still belong to the discipline and to science.

Of course, a qualified and competent scientist is quite likely to have a large repertoire of scientific beliefs. Elementary scientific education is particularly designed to inculcate belief. However, at lower levels, I would claim that what is required is more enthusiasm for science than belief in specific conclusions. Certainly, most students quickly forget many of the conclusions they are taught. In the process of his subsequent training, a scientist will have had to make observations and to follow chains of reasoning which were selected so as to make it easy to believe in the central scientific dogmas of his discipline. However, during his more advanced training, he was not required to believe but simply to display certain kinds of competence -- being able to use a technical language appropriately, to do certain kinds of experiments and solve problems. He had to be familiar with a range of acts and theories rather than hold any ideas as true. The observant student is quite likely to be increasingly cautious about making commitments of belief. He learns that his own observations cannot always be trusted, especially if they lead to unorthodox results, even if he was following a standard procedure. But orthodox results are not fully trustworthy either. The student cannot help noticing that textbooks are very often out of date, and that although teachers may be more up to date, they may have idiosyncratic doubts and enthusiasms. If he takes a broader view, he is likely to come to the conclusion that the ideas of science have a limited period of currency.

Once a qualified scientist begins to practice, he will, of course, normally make some sort of commitment of belief to those matters which he regards as underlying and giving sense to his practice. Such beliefs are important in motivation. The precise formulation and the extent of such underlying commitments will vary with the individual. It is psychologically important to believe in the truth of what one is creating as new science. At the point of discovery, scientists tend to be realists, thinking of themselves as uncovering new secrets of nature. At the laboratory stage of the process, they naturally have initial uncertainties. But studies in the sociology of scientific knowledge suggest that any initial doubts are likely to be suppressed in favor of the representation of conclusions as objective facts as the campaign mounts of getting the new science accepted by others. (See, for example, Latour and Woolgar, 1986.) In contrast, the creative commitment of belief is not necessarily shared by those to whom new scientific findings are addressed. Many of those who are exposed to new scientific ideas will take a sceptical attitude as a deliberate role. But to diffuse successfully, an innovation normally has to reach people who are enthusiastic enough to try it out, and be sufficiently successful with it to spread it further. Within its self-created wave of enthusiastic use, committed belief can build up. But as soon as problems emerge, comments will tend to shift back toward doubt and caution. People will once again disagree in the extent of their expression of realism. In the longer term, a lasting scientific conclusion will be repeated in textbooks and taught to new generations of students. The case for its acceptance as belief is put in an objective form, designed to generate belief in further generations of students. But in the longer historical time scale, it is generally accepted that some of the apparently immutable truths of science are likely to be overthrown, others will become hidden behind a maze of qualifications and even more will become obscure and redundant.

As a philosopher of science, I used to wonder about the variability of epistemic commitment displayed in the professed beliefs of scientists. In one context they may sound like realists, in another extremely cautious and sceptical. In the present discussion, I want to exploit this sense of the variability of commitment to belief by saying that it is simply a consequence of the fact that scientists are not *required* to believe, that beliefs are a matter of personal judgement. There is not even a requirement of consistency of belief. The culture of professing beliefs in science is not rich enough to provide clear rules, so that people tend to say what sounds right in each context, without worrying about consistency. There would be something bizarre about going into scientific laboratories and asking the

inhabitants what, in their role as scientists, they were committed to believe. The bizarreness comes, in part at least, from the modern view of science as producing a changing repertoire of findings rather than permanent contributions to knowledge.

Kuhn's account of normal science was couched in a language of faith and commitment which could have been applied quite easily to religion. However, this aspect of Kuhn's account applies best to rather small communities, those which have the cohesiveness of a scientific *school*. The band of followers of a charismatic leader in science frequently display an immense loyalty to the school's core ideas, and a messianic zeal in spreading those ideas more widely. If such a school is at the heart of a new paradigm of scientific research, Kuhn's account is plausible. But as a school grows, its distinctive findings become increasingly qualified and its ideas diluted by other currents of thought and practice, middle age sets in, the zeal fades, and with the eventual death of the leader, enthusiasm can turn into the public expression of caution. Scientific schools tend to live in controversy, so that such organized commitment is normally a feature of a scientific faction rather than of a science, and at best is merely a phase in the cycle of development of a discipline.

The zeal of a committed scientific school has some of the appearance of a religion or pseudo-science to those who stand outside it. Consider the following lines of doggerel, which were applied to the theory of ionic solution as developed by Wilhelm Ostwald's new school of physical chemistry which was pioneering a mathematically and physically more sophisticated approach early in this century.

> *" I used to think theology*
> *was rather tough on doubt,*
> *But chemistry with ions beats*
> *Theology all out.*
>
> *You'd better join the church before*
> *Your course is well begun,*
> *Because you'll need to exercise*
> *The art of faith, my son."*(5)

These lines imply that a high level of faith might be appropriate for religion, but if it occurs within a scientific faction it is a matter for ridicule. Such commitment is not desirable in science.

In summary, then, such commitment of belief is heuristically important at certain phases in the development of science. But although individuals regularly confess to having beliefs in science, they tend to change their minds about what beliefs they hold and no one can ever be held to any such belief as a scientist.

Belief in Science and Pseudo-Science

Although there is no formal requirement to believe anything in particular in orthodox science, the situation is less clear for the forms of unorthodoxy that scientists may wish to reject as pseudo-science. Sometimes an unorthodox quasi-scientific practice has the appearance of a religious sect, requiring full commitment from its supporters. Sometimes a form of unorthodoxy can seem like a school within science. Sometimes the contrast between a religious sect and a scientific school can blur. It can appear to outsiders that the requirement of uncritical faith in the claims of a form of unorthodoxy makes it more likely that it is pseudo-science. Throughout its history, the psychoanalytic movement seems to have required fierce loyalty to a number of its core beliefs. This has the effect of making crucial features of the doctrine far more rigid than any natural science, more like a religious faith. However, not all forms of scientific unorthodoxy require such commitment from all who are interested. The forms which flourish within popular culture, such as astrology or parapsychology or UFOlogy, often survive on the mild curiosity of a large number of individuals, who provide a literary market and a recruiting ground for the more serious. In a practice like astrology or parapsychology, the professionals are likely to profess profound belief. But they could practice almost as well if their interest was purely commercial or even fraudulent.

If the view argued here is correct, an explanation is needed of why belief is not an institutionalized requirement of modern science. It is a result of the changes between the seventeenth century and the twentieth century described earlier in the paper. First, because an individualistic epistemology still plays a key role in our culture, it is up to the individual to make the judgement of what he believes. No one else can force him to accept what he does not want to. Because of the complexity of the process of expertly going from trained observation to technically reasoned conclusions and because of the admitted revisability of the final conclusions, it is not merely outsiders who may disagree with any given result but scientists themselves. Indeed, professional scientists might be said to have an interest in never finally settling any issue -- because if they did, then that would be one less thing they could continue to research. As far as I know, that professional interest in leaving all questions open is *never* rejected.

For ordinary purposes, belief, commitment and knowledge are closely integrated. If scientists wish to use their science as a form of authority, they must link it into the ordinary discourse of belief and knowledge. But in the process of research, it is a psychological and social requirement that the close integration of existing science with the rest of common sense be broken. The impetus to producing new and original science cannot be satisfied by merely adding to existing knowledge. Existing science must, on occasion, be modified.(6) In this context, science is not a form of permanent belief, but an instrument for its own enrichment. This instrument is commonly regarded as becoming an ever more powerful resource for our culture and our practical activities. The scientist must be very careful about declaring belief or commitment to preventing their change. There is, then, an institutional tension within the social system of science between linking science authoritatively to everyday belief and presenting an attitude of receptiveness to scientific change. This corresponds to the tension noted in main stream philosophy of science. On this view, the only remaining institutionalized requirement for individual belief in science may be belief in the collective integrity and competence of one's fellow scientists. Any individual or group who refuses to recognize the authority of those people has to face the social consequences of that refusal.

Perhaps the difficulty with the application of the concept of belief is that it is so readily tied to the beliefs of the individual. We might move to a consistent *social* epistemology.(7) No longer would an individual have to be persuaded by seeing for himself the truth of a claim. Instead, we would think in terms of a coherent system of beliefs having the authority of those who collectively authorize them.

The Problem of an Authoritative Rejection of Pseudo-Science

I will now argue that although it might be in an individual's interest to reject pseudo-science, he cannot definitively use the authority of science to do it. First, following from the previous discussion of scientific belief, there are no items of knowledge authorized by science on which he can build an argument. There are no authorized facts, no authorized laws, and no authorized theories. Secondly, I will go on to argue that there is not even an official methodology of science that scientists are required to believe that can be used as the basis of a rejection of a challenge from unorthodox science.

Rejection on the Basis of Scientific Conclusions

Anyone who wishes to reject as pseudo-science any set of unorthodox ideas and practices claimed to be scientific and who wishes to give rational authority to the rejection on the basis of the conflict with orthodox science must go through the same sort of process for the induction of belief which is employed in persuading students to accept standard science and in persuading scientific communities to accept new science. But it must be done in a way that is persuasive to the non-expert. From what I have already said, it will be clear that this process cannot be definitive. The non-expert can always point to the leaps of faith in modern scientific reasoning which the committed are prepared to make but the sceptical are permitted to baulk at. The non-expert can simply say 'Well, I think scientists would say that, they are biased towards uncritical acceptance of orthodoxy.' An ingenious defender of unorthodox views will find that there is no end to the rational strategems which can be employed to keep the orthodoxy at bay. I have tried playing this game myself as an exercise for students. I go into a pre-warned class of undergraduate science students and say that according to *my* textbook, the earth is flat.(8) Can they prove me wrong on the basis of their own observation and reasoning? I can get away with it because I know enough tricks to evade refutation. All I have to do is to introduce a series of increasingly drastic adjustments to the rest of science to keep the flat-earth option alive. Faced with such ingenuity, there is no simple way for the defender of scientific orthodoxy to keep to his rational use of authority. When he hears what I, in the role of a flat-earther, propose to do to optics, to the theory of the magnetic and gyroscopic compass and even to the topology of space if I am forced into it, and then how I appeal to a conspiracy theory of the fraud of space travel, he is likely at some point to abandon the attempt at rational argument. At that point, he will have to retreat to a balder assertion of authority. He may say that if the defence of belief in a flat-earth has to reject *so* much of orthodox science for no other reason then the committed flat-earther must be beyond reason. He may even try to suppress anyone who tries to spread such foolish ideas. In other words, when rationality fails, he must retreat into the more basic stance that we must choose between the coherent and sensible tradition of the scientific orthodoxy and the foolish and unproductive tradition of the flat-earther.

If you think that the flat earth example is rather silly, because people could only defend a flat earth in modern society out of ignorance or as a joke, perhaps

another example will help to make the point. Consider, for example, the impossibility of perpetual motion. The idea of a machine that can run forever without an external source of power has traditionally been attractive to practical inventors, and to the more innocent people who can be conned into giving inventors money. A worthwhile perpetual motion machine would not merely run forever, but actually do useful work forever, without needing power to do it. Now, although men of science were not always clear about such matters in the early years of the scientific revolution, it has seemed increasingly obvious to the scientific elite that such perpetual motion is impossible. The inventors of the eighteenth century were telling each other what a good idea perpetual motion was, and were telling stories such as that about Councillor Orffyreus, who had invented an operational perpetually revolving wheel, but had destroyed the machine because he was having so much trouble keeping it secret. (See, for example, Smedile, 1962, p. 12.) But at the same time, orthodox scientists were building scientific theories on the *presupposition* that perpetual motion was impossible. The evidence in this case is that those scientists who tried to discourage inventors were *not* able to give a convincing rational case. Although some specific limited forms of perpetual motion, such as an overbalancing wheel powered only by gravity, can be shown to be incompatible with specific theories, science has never been able to offer a fully general argument for the impossibility of perpetual motion beyond the claim that it has never been convincingly demonstrated. What has happened instead is that such branches of science as thermodynamics have been constructed around the supposition of the impossibility of perpetual motion machines of particular kinds. Of course the possibility of perpetual motion is given no credence whatsoever by orthodox science, but that is not enough to *show* that it is impossible.

Rejection on the Basis of Methodology

So far, I have mainly talked about the use of the findings of science to give authority for the rejection of pseudo-science. There is, however, an alternative rational strategy, and that is to appeal to a theory of scientific methodology. In the context of a discussion of the science and pseudo-science of creation, I argued that there are a large number of theories of scientific method which are defensible today. (Dolby, 1987.) My list of prescriptive criteria included theories of good induction, the operationalist criterion of adequate scientific concepts, the logical positivist criterion of rational reconstruction within a fully verifiable system, Popper's falsificationist criterion, Lakatos's criterion of progressive problem shifts in research programs, and several others. If there was only one orthodox theory of the nature of science, then there might be some hope of persuading people that whatever was done in the name of science would have to conform to that methodology if it was to avoid being identified as pseudo-science. But since there are so many methodologies, and since each can be used as the basis of effective criticisms of the others, there is no simple way at present in which the sophisticated defender of unorthodox views can be shown to be wrong. He can either say that he is following the methodology proposed, or that that is an inappropriate theory of scientific procedure and that he is following a more appropriate one. He could even invent his own theory of science as some Marxists did in saying that the proper method of science is dialectical materialism. Again, there is no authority in appeals to rationality in terms of which the pseudo-science can be rejected authoritatively. The only effective measure for the orthodox is to abandon the rational game at the point at which it becomes clear that the unorthodox opponent can use it effectively in his or her own defence. Coercive measures best understood in social, political and economic terms are all that remain. The scientist can exploit the power and privilege of his position within the lower levels of the establishment of society, and refuse to treat the unorthodox competitor fairly. The moves available to the scientist are not so different, then, to those employed in religion in acts of excommunication.

In religion, heresy involved the abandonment of a required belief or procedure. Those who committed it could be excluded from the rank of believers. In science, it is difficult to institutionalize comparable excommunication procedures. They must be left to the informal action of the individual. However, when many scientists share the view that some system of belief and practice is pseudo-science, the defender of the view they reject will find himself without access to any of the resources of science -- such as finance, specialist communication, social support, and so on. The unorthodox can only survive by taking rival social measures and founding a new, rival movement.

Conclusion

Science does not after all provide a decisive rational authority for the rejection of defended pseudo-science, even though scientists are often concerned to reject such practices. Furthermore, there is a trend in our culture which may well make it increasingly difficult for scientists to defend their interests by non-rational methods of rejection of alternatives as pseudo-science.

Our culture has become ever more diverse, with far more resources than can be assimilated by specific individuals or small groups. There is a tendency for rootless individuals to pick and choose the cultural objects that take their personal fancy. The old orthodoxies of the cultural citadels of past society may well find it harder to maintain their authority if the social goals of individuals become more divergent. Even the established religions feel the pressure as members of each new generation seek new and increasingly diverse syntheses of insight as the route to salvation. The trend towards cultural plurality may become unstoppable as the media of communication transcend old cultural barriers. In the age of international and satellite television, we are increasingly exposed to images showing how other people live and what they value. The trend towards cultural plurality is also being driven by the cumulative effect of professional specialization and differentiation. It is easiest to achieve excellence in such a diverse culture by becoming master of a very narrow topic. But then, one's primary social orientation is likely to be towards the small proportion of people who share an interest in that topic. The institutions of science might withstand the fragmentation of the external culture, but the same trends are at work within the scientific culture itself. The scientific enterprise itself is now so vast that cross-disciplinary communication is normally far more superficial than that within disciplines. Rival scientific enterprises can ever more easily emerge in separated disciplinary or multidisciplinary frameworks. The kinds of conflict of interest that can arise between science and pseudo-science may increasingly arise within orthodox science. If I have spotted this trend correctly, the scientific orthodoxy may increasingly lose its monolithic nature.

The vision I am offering is of many networks of collective judgement, organizing and reorganizing, none definitive and none fully authoritative. In such a system, critical judgement remains possible. For example, the growth of present science is powered most effectively by its being patronized as a resource in the various competitive struggles in society. As states struggle for military dominance or for prestige, they pay for the kind of science which turns out to give the greatest competitive advantage. As business firms seek economic advantage, they draw upon such cultural resources as science and technology which actually benefit them in their struggles. As individuals seek to advance their careers in such systems, they seek the channels which will work best for them. In all these competitive struggles, specific sciences and specific alternatives to science must show themselves to work. In a more pluralistic society, with more diverse loci of power, such competitive struggles may increase. Alternatives to orthodox science can survive to the extent that they can get such patronage. At present, the popular media are one place where orthodox science cannot win exclusively. In the search for what is interesting and newsworthy on a human scale, it turns out that stories about psychic phenomena, or UFOs, or whatever, provide a welcome alternative to orthodox science. The media are in competition, and orthodox science lacks the power to control that competition in its own interest.

Notes

1. The title of Popper (1974) summarizes this aspect of his work most clearly. Similarly, the title of Kuhn (1970) highlights the same point in his work.
2. M. Polanyi (1967) gives this answer in providing scientific authority to the rejection of Velikovsky.
3. The weakness of the sociological use is typified by G. Böhme (1986) in this remark: "For sociological purposes, I have defined knowledge as participation in the cultural wealth of society" (p. 65).
4. In the conference at which the earlier version of this paper was presented, it was claimed that commitment to religion was more like making a personal journey, and that the beliefs one held could be expected to change as one continued on that journey. The main commitments, it was claimed, were belief in *individuals* (such as Christ), rather than in the truth of propositions. However, as long as such a journey is to remain within a particular religion, I would suggest that a certain core of belief is required to remain constant. Furthermore, the belief in Christ depends on belief in the truth of the central claims of the tradition stemming from Christ, in particular the documents which became the New Testament many decades after the events they record.
5. Ellwood Hendricks, as quoted by James Kendall (1925).
6. Popper and Kuhn emphasize this feature of science, but neither are very clear about the precise boundary of that which may legitimately be modified in the research process of science. Popper was peculiarly unclear in these details of his fallibilism. He regards the background knowledge required for the test of a new theoretical conjecture as unproblematic. And yet, by his own precepts, no background knowledge can ever be regarded as more than provisionally held and at risk of falsification. Kuhn regards the coherent unit of science as that which is shared by a specialist community. It is this shared specialist knowledge which the practitioners regard as negotiable, and the knowledge coming from other specialisms is normally accepted without question, or at least as outside their competence.

7. As proclaimed, for example, in the journal Social Epistemology: A Journal of Knowledge, Culture and Policy, 1987-.
8. For example, I pass around 'Parallax' (1873).

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